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BIBLIOGRAPHICAL NOTICE.

ACUTE HYDROCEPHALUS, OR TUBERCULOUS MENINGITIS OF CHILDREN.

I.—*Dictionnaire de Médecine*, Vol. XIX.

II.—*Archives de Générales de Médecine*. September and November, 1839.

IN a late number of the Medical Examiner, we noticed the excellent Dictionary of Medicine, now publishing at Paris. In the nineteenth volume is contained an article on Meningitis by Dr. Guersant, which we noticed with commendation, at the same time we stated that, in some respects, it was unjust and exceptionable. In describing the disease, the author has necessarily spoken of the history of the affection, which has been completely misunderstood until quite recently, and, while he alludes to the researches made at the children's hospital by one of the Editors of the Examiner in conjunction with Dr. Rufz, and, in fact, admits that these observations were the first to establish the real character of the disease, he introduces, in the text, the assertion that these observations were not, in reality, original, and if they were original, that another had preceded Drs. Rufz and Gerhard in this matter. The latter remark is the more objectionable, because it is not positively said, but is rather introduced as an inference from the context.

This injustice is of little importance as a purely personal matter, but it is nevertheless natural that some value should be attached to a discovery which, although comparatively limited, was only obtained at the sacrifice of much time and labour; and on the other hand, the general interests of science require that the authorship of a medical discovery should belong to those to whom it is justly due. It is, besides, a matter of undoubted right; and an attempt to take away the credit attached to the discovery of a scientific truth from those to whom it belongs, is quite as great an injustice as any other interference with the right of property. We have been accustomed to respect the character of M. Guersant, and we are not disposed to censure him for an error which we presume is involuntary; nevertheless, it is a

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matter of duty on our part, to place the subject in its true light, and to sustain the manly and honourable course of Dr. Valleix, the editor of the Paris "Archives," by a simple statement of facts which we shall publish in the next number. The connection of the subject with the history of many important diseases both of infancy and adult life, must be the best apology to our readers, for the length of the papers. The following extract includes the portion of the article, Meningitis, to which we have referred.

"But what is most important to notice, is that this variety of meningitis presents constant alterations and anatomical characters very different from those of ordinary meningitis. Among these, is one which merits particular attention; this is the granulations which are observed in the sub-arachnoidal tissue. Thomas Willis is, perhaps, the first who ever noted this alteration as being very important. He says in his work, *De Anima Brutorum*, published at Amsterdam in 1682, pars Pathologica, p. 119: '*Nec minus à phlegmone et abcessa, quam ad hujus modi meningitis modis et tubercularis nonnumquam cephalalgia, lethalis et incurabiles oriuntur.*' This observation of Willis had been forgotten; nevertheless, most authors who have written on hydrocephalus, have spoken of meningeal granulations, but without attaching much importance to them, and without connecting this alteration with the tubercle degeneracy. I have, however, long been struck with this remarkable coincidence, that in meningitis with effusion in the ventricles, we find, together with granulations in the meninges, tubercles, either in the bronchial ganglions, or in the lungs; so that in my clinical lectures, I considered hydrocephalic children as phthisical patients who died by the brain, (considerations sur plusieurs maladies des enfans, thesis of M. Leth, Paris, 1839.) I believed, for this reason, that I could already separate meningitis with granulations, from other kinds of cerebral inflammation, and I had assigned it the name of granular meningitis ever since the year 1827, as shown by the registers used in the hospitals during this year, but I had not yet considered, at this period, those granulations to be true tubercles. Later, Dance, in his '*Memoire sur l'hydrocephale*,' did not hesitate to approach these granulations to the miliary tubercles found in the pleura and the peritoneum. This truth, which he only announced, has since been placed beyond doubt by the researches of MM. Rufz, W. W. Gerhard, and Constant, who

attended, at the same time, the hospital 'Des Enfants Malades.' M. Rufz, who was then attached to the service of my division as 'interne,' has published in the Archives Générales de Med. of February, 1833, several observations, in which he points out the granulations. Later, he proved that those granulations, which are met with in most cases of acute hydrocephalus, are of a tubercular nature, by availing himself of new facts in the thesis, which he sustained in 1835, before the Faculty of Medicine. M. Gerhard, (Amer. Journal of Med. Sciences, No.'s for February and May, 1834,) gave a series of observations, made in the 'Hôpital des Enfants,' on cerebral affections, among which, the greater part belong to the disease which now occupies our attention, and which he also considers as a tubercular meningitis. The unfortunate Constant, an exact and conscientious observer, taken away too soon for the science, who was occupied in the same researches as MM. Rufz and Gerhard, and to my knowledge, before them, presented in 1835, together with Dr. Fabre, to the Academy of Sciences, a very good monograph on tubercular meningitis, which procured them the honour of the Monthyon prize (?) An extract from this monograph, the source of which the author does not indicate, has been published by Dr. P. N. Green, who had acquired the manuscripts of Constant. This extract, inserted in the Lancet, May, 1836, was afterwards translated in the first volume of the 'Encyclographic des Sciences Medicales.' An excellent thesis, published in the same year by M. Piet, then an 'interne' in the 'Hôpital des Enfants,' contains a just appreciation of the works of those who preceded him, and some observations on tubercular meningitis, peculiar to the author himself. M. Coignet and M. Becquerel, each in a thesis presented to the Faculty of Medicine in 1837 and '38, give a summary under different forms, of the works of their predecessors in tubercular meningitis. Again, M. Le Diberder, in his thesis on the tubercular affection of the pia mater in adults, and M. Valleix, in his paper on the same subject, inserted in the Archives, in the month of January, 1838, establish, in an incontestible manner that the acute hydrocephalus of adults depends on the same tubercular alteration as with children, and presents the same characters.

This pathological truth, appearing now to be well proved, it is then necessary to establish two sections in the history of meningitis. We will give to one the name of *tubercular meningitis*, as being more precise than that of *granular*, we at first adopted, and to the other, that of *simple*, or *non-tubercular meningitis*. This distinction is founded on considerations of pathological anatomy, well determined, and so much more important, that they coincide, as we shall see, with the physiological disorders, often enough to render it possible, in the greater number of cases, to recognise and distinguish

these two diseases, which offer, otherwise, as to prognosis and treatment, very different consequences. These two kinds of meningitis have until now been confounded, in nearly all the works that have treated of these subjects. The monograph of MM. Parent and Martinet contains, in particular, nearly twenty observations of tubercular meningitis in children, and nearly as many in adults, so that nearly a third of the observations relate to this disease."

It gives us great pleasure to observe that justice is rendered in this matter by the French journal of greatest authority, we mean the Archives of Medicine. The following remarks are found in the analysis of the Dictionary, published in the number of the Archives for September last, relative to the article Meningitis, by M. Guersant.

"The article Meningitis was vague, and could not be otherwise, when the first edition of the Dictionary appeared; but since then, the exact observation which is now required, has caused this important point of cerebral pathology to be greatly advanced, and it is with the new materials collected within the last few years, that M. Guersant has made the principal additions to the original article. Here we are obliged to enter into some details, because, in relation to the history of the researches on the meningitis of children which have been made at the children's hospital, we cannot agree with the author of the article.

"Every body, until quite lately, attributed to MM. Rufz and Gerhard, the discovery of this form of meningitis, which is so frequent and so fatal in young children. In fact, the former had published in 1833, in the Archives, several observations, in which the existence of tuberculous granulations in the membranes is placed beyond doubt. The latter inserted an analogous memoir in the American Journal (of the Medical Sciences) in 1834, and, finally, M. Rufz presented his thesis, the subject of which is Tuberculous Meningitis, in February, 1835. During all this time nobody disputed their discovery; but lo and behold! quite recently, M. Fabre in his name as editor, and in that of M. Constant as observer, claims the priority, and sustains that the meningitis was known by them before MM. Rufz and Gerhard had begun their researches. M. Guersant seems to sanction their pretensions in saying, in his article, 'The unfortunate Constant, an exact and conscientious observer, carried off too early for the science, who was occupied with the same researches as MM. Rufz and Gerhard, and to my knowledge before them, presented in 1835, in concert with M. Fabre, to the Academy of Sciences, a very good monograph on tuberculous meningitis, which gained for them the Monthyon prize.' These are very late claims. How can we imagine that an observer who has made his researches before all others,

should allow, without complaint, during entire years, the publication of memoirs which carry away the honour of his labours? The fact is so improbable, that we should not hesitate to think M. Guersant was deceived by his recollections, if even we did not positively know, that not only M. Constant had not preceded MM. Rufz and Gerhard in their observations, but that he did not recognise the granulations of the pia mater, until they were pointed out to him by the pupils of the children's hospital, who had all seen them before him. We certainly desire to show all possible respect to the memory of an industrious young physician, but we cannot endure that credit should be given to him for what eminent observers have gained by painful exertion, and we do not doubt that he himself would oppose it, if he were still living. Besides, in questions of priority, nothing should be admitted as testimony but the date of publication, that is, when each has had time and opportunity for complaint, as is the case at present. To admit other principles, would introduce trouble and confusion in literature, and open the way to the most strange pretensions."

To these observations M. Guersant thought it necessary to address the following answer:

To the Editors of the Medical Archives.

Gentlemen,—Permit me to claim the insertion in your next number, of the letter which I have the honour to address to you, in order to correct some errors, which have doubtless been overlooked in the hasty preparation of the notice of the nineteenth volume of the Dictionary of Medicine, article Meningitis. Your collaborator makes me say things which I have not said, and passes in silence that which I had explained with sufficient clearness. I am under the greater obligation of addressing this protest (reclamation) to you, as it concerns several of our brethren, both dead and living, and is not confined to myself alone. By a singular preconceived idea, the author of the notice of the article Meningitis, has made the distinction between the two kinds of date from 1833. However, in the short historical sketch which I have given of the progress of the science relatively to this part of the pathology of the brain, I have shown that the origin of the distinction now admitted between the simple and tuberculous meningitis dates back as far as the description of acute hydrocephalus, by Robert Whytt and André de St. Clair. As to the granulations of the pia mater, the discovery of which is attributed by your collaborator to M. Rufz, they were already known by Willis in 1662, as I have proven by a passage of that author which I have cited word for word, and they have been pointed out, and more or less completely described by several of the modern authors who have written on acute hydrocephalus or the meningitis of children. They are well described in an observation of M.

Cruveilhier, which is inserted in the work of M. Bicheteau on acute hydrocephalus. The observations VIII, XI, and XIV, of the work of M. Charpentier, contain a good description of it; and our late Colleague, M. Dance, in his memoir on Hydrocephalus, printed in the Archives, says, in speaking of these granulations, that they are of a tuberculous nature, and altogether similar to those met with in the pleura and peritoneum. For myself, I had often ascertained the existence of that kind of alteration at the children's hospital, as the registers of cases which are deposited in the office of administration of hospitals will show, before the observations of the persons alluded to. The register of the year 1824, kept by my son, then my interne, includes several observations of meningitis in which the granulations are very well described; I have pointed them out as one of the pathological alterations met with in meningitis, (Dictionary of Medicine in twenty-five volumes, first edition.) Having observed them more frequently since that period, I had established, in my course of clinical medicine, the distinct nature of granulated or granular meningitis, and I pointed out the fact that children who succumb under this affection present always tubercles in the bronchial glands or the lungs, and are really nothing but phthisical patients, who die of the affection of the liver.

Such was the state of our knowledge in 1833, when M. Rufz published his first observations on acute hydrocephalus, and more recently in 1835, his thesis on the same subject. In giving an account of these observations, of those of M. Gerhard, and of the monograph of MM. Fabre and Constant on tubercular meningitis, I said, *that to my knowledge*, M. Constant was occupied in researches on this subject, and it is a fact well known among those who attended the children's hospital, that M. Constant collected observations on affections of the brain, attended the clinique, and had consequently seen these granulations of the pia mater, which were observed every year, even before M. Rufz was my interne. I have, then, only advanced a fact which is known to all those who frequented the hospital; but I could not attribute to M. Constant, as your coadjutor asserts, the priority of the discovery of granulations in the pia mater. I do not even know that these gentlemen have ever raised a claim which would not, indeed, have a better foundation than that of M. Rufz, since I have carried back the modern discovery of granulations one hundred and fifty years; and hence those granulations of Willis, forgotten for so long a time, were again brought to light by the researches of many physicians and pupils of the hospitals, more than eight years before M. Rufz claimed their discovery for himself. Though I accord to the labours of my young colleagues the merit which is due to them, I have thought that in order to be just to all, I

ought to render up both to the dead and the living what rightly belonged to them.

M. Valleix's answer to the Reclamation of M. Guersant.

I regret very much that there should have been a misunderstanding between M. Guersant and myself. I never doubted that M. Guersant had some ideas of the existence of sub-arachnoidal granulations. I even admit, although one might reasonably have some doubts on this subject, that these ideas are traceable as far back as Willis. I, myself, in an article in this journal, cited some observations of Dance, in which this morbid production is described. But, as M. Guersant has well remarked, all of these ideas were entirely insufficient. They were so in this respect; that he, himself, who had studied the question better than any one else, did not hazard his opinion on the value of these lesions, and their relation with the symptoms. We were then compelled to say, that, in some badly determined cases, tubercular granulations were found in the pia mater; but there was a wide distance from these rudiments of knowledge, upon an important pathological question, to an exact description of the symptoms, progress, termination, and characteristic lesions of the disease. And it is right to say, that he who has attained this last end, is really the discoverer.

It is, then, on this part of the subject, that it has appeared to me that the question should be made clear, and I have asked myself, to whom, whether to MM. Ruz and Gerhard, or to M. Constant, belonged the honour of what I believe to be a discovery. Well, I think that if the first researches of M. Constant, which M. Guersant has mentioned in his paper, only led him to the same results as those of his predecessors, and if it was not until after having read the statement of MM. Ruz and Gerhard, that M. Constant saw the accompanying symptoms of a lesion, which he sometimes met without knowing the value of, the constant occurrence of this lesion, the extreme rarity, to say nothing more, of the truly inflammatory meningitis of children, the inflammatory meningitis which was regarded as the most frequent disease before the researches of these two authors, if all of these suppositions are just, and I have the strongest motives for regarding them as such, I believe that I do no one wrong, when I attribute to MM. Ruz and Gerhard the discovery of tubercular meningitis. Before the time of MM. Petit and Serres, ulcerations had been found in the intestines of persons who had died of low forms of fever; the symptoms of these fevers were also known, but no one will hesitate to say that they have rendered us familiar with entero-mesenteric fever; and why? because before them, the relation of constant lesions with certain invariable symptoms, had not been noticed.

I regret having entered so much into detail, but it was my desire to prove that there was no anticipation on my part, and that my intention was most certainly not to throw a veil over the researches of M. Guersant, to which I have always been desirous of rendering justice. If I explained myself badly in my bibliographical article, I hope that this will dispel all uncertainty. My reading on the subject in question, has raised in me the conviction that it was not right to concede the priority to M. Constant; this is all that I wished to say, and I leave to the parties interested, the care of carrying the discussion farther, if they judge it necessary.

VALLEIX.

CASE OF POLYPUS UTERI, Successfully removed by ligature and excision. By JACOB G. WALKER, M. D.

To the Editors of the Medical Examiner.

Leasburg, Carswell Co. (N. C.) April 24, 1840.

GENTLEMEN:—The following case of uterine polypus, extraordinary for its magnitude, (weighing four pounds, eight ounces,) is communicated to you, for insertion in your valuable periodical, which, if you think worthy of such, is at your disposal.

Yours, very respectfully,

JACOB G. WALKER.

Mrs. S—, the mother of five children, the youngest of which, about four years old. Some time after its birth, she experienced more or less symptoms of local irritation in the neighbourhood of the womb, unaccompanied for a considerable period with any signs of tumefaction or visible manifestations of disease. Ultimately, however, about two years since, (or the rise,) there appeared upon examination, by Dr. J. Comer, (then her physician in charge,) an oval tumour, the size apparently of a hen's egg, who, upon continuing his examination, (per vaginam,) suspected the existence of polypus, and upon disclosing the same, and the probable necessity of committing her case to the doubtful, but sometimes efficient operations of nature, desisted from further treatment than that of a mere palliative one. But after the lapse of some months, the rapid growth and augmentation of the tumour, and consequent distention and irritation communicated to the uterine vessels, had the effect of exciting periodical hæmorrhages at intervals, recurring every two or three weeks, which, for the time, would soften and relax the tumour, immensely, accompanied with symptoms of great prostration and exhaustion, peculiar to sanguineous discharges.

This course of hæmorrhagic visitations, has been kept up (by the phenomena and operations of nature) pretty much for the last twelve or eighteen months, during which time the writer of this article has had frequent opportunities of witnessing and examining the various

stages of growth, in the development of this frightful and malignant disease; and has no doubt, but that the growth has been greatly retarded in consequence of the frequent losses of blood, notwithstanding they have had the effect of greatly impairing and undermining the patient's constitution.

However, about two or three months previous to the removal of this tumour, there was a suspension of hæmorrhagic discharges, succeeded by an increased rapidity in growth, to that degree, that the uterine cavity and vaginal canal were literally blocked up with partial protrusion beyond the os externum, the abdominal tumefaction almost equalling that in the last stage of utero-gestation, until, on the night of the 14th instant, she was suddenly attacked with the usual symptoms of labour-pains, with unusual violence, which continued unabatingly until I was sent for; and found upon arrival, that the tumour had presented near one-third; and, from its distension, there was a complete suppression in the urinary and fæcal discharges, while in attempting to introduce the catheter, it was impracticable to find the mouth of the urethra, and the duration of the urinary suppression, the consequent distension of the bladder, contributed much to the aggravation of existing symptoms.

Under these circumstances, the entire suppression of urine, the impracticability of drawing it off, being unable to push back the mass, we concluded that there was no alternative but death, without the removal of this fungous growth; therefore, Dr. J. Comer was called in, who agreed with me as to the propriety of undertaking the operation, which consisted in a gentle but persevering traction at the protruded portion, co-operative with the uterine contractions, which were very active at this period, until at length we succeeded in delivering the principal bulk of the tumour, clear of the vagina and os externum; finding it impossible to advance any farther, we determined on applying the ligature as high up the pedicle as possible, after which, we excised the tumour below the ligature, and subjected the same to weight, which proved to weigh four pounds eight ounces, or perhaps five pounds, including the neck.

The woman is, at the above date, apparently doing well, and advancing towards convalescence.

FOREIGN CORRESPONDENCE.

LETTER FROM PROFESSOR MARTINS.

No. V.

Examination of some recent investigations on the Capillary Circulation.

PARIS, 13th March, 1840.

To the Editors of the Medical Examiner.

THERE is, to the physician, no more interesting chapter in all physiology than that which

treats of the motion and modifications of the blood in the capillary vessels. It is here that take place all the great phenomena of inflammation, and of the transformation of tissues which constitute two-thirds of the more serious diseases with which mankind are affected. Important researches have recently been made on this subject, by Messrs. Müller of Berlin, Weber of Leipsic, Huschke of Jena, and Poiseuille of Paris. Having had the good fortune to see their preparations, and to assist at some of their experiments, it has occurred to me that I might afford some interest to your readers by presenting them to their notice in this letter.

The diameter of the capillary vessels varies, according to Messrs. Müller and Weber, from 0.00025 to 0.00050 of a line. This diameter is smaller than that of the thinnest excretory ducts of the kidneys, mammae, salivary glands, and testicles. The closest network of vessels is found in the lungs, and the choroid coat of the eyeball. While every where else, the intervals separating the vessels have a diameter superior to that of the vessels themselves, it may be asserted, that here the diameter of the intervals is equal or inferior to that of the vessels. It has been a subject of considerable discussion whether the fact of the serous membranes having received from the blood vessels the injections of Blenlaud, has placed this point beyond doubt. The experiments of Messrs. Müller and Weber have established two facts which are not of less importance. The first of these anatomists has shown that all the excretory ducts of the glands terminate in a cæcum, or cul-de-sac, and that there exists no communication between them and the capillaries, as Haller had supposed. The second has demonstrated that there is no anastomosis between the capillary vessels of the placenta and those of the uterus in the pregnant female; that the circulation of the fœtus and that of the mother are not identical, but simply in contact; and, that the passage of the blood from one to the other takes place only by imbibition. I have had the advantage of seeing the numerous preparations which prove that at the moment of the separation of the placenta there is no solution of vascular continuity, and that the hæmorrhage is not owing to the rupture of the vessels.

M. Poiseuille, in a memoir which received the prize at the Institute, has investigated the

causes and phenomena of the movement of the blood in the capillary vessels. He at first examined the movement of this liquid in a part separated from the influence of the heart, either by a ligature or by removal from the body of an animal. The following are the facts discovered by him: If, by means of two weights, a portion of a capillary artery or vein be isolated in the mesentery of a frog, so that about half an inch be comprised between these two weights, the circulation is interrupted; then these vessels contract gradually upon themselves, owing to the elasticity of their parietes. Their calibre, therefore, just as that of the large arterial trunks, is owing to the pressure of the blood from within, which distends them in every direction. Now, if the femoral artery and vein of a frog be prepared for the space of half an inch, and, if by means of a strong ligature, all the parts of the thigh be enclosed, except the nerves and blood vessels, it will be seen, by means of the microscope, that the circulation continues to take place in the interdigital spaces. Let the course of the blood be then suddenly arrested in the femoral artery alone, and the globules of blood will be seen to move slowly in the capillaries, and stop at the expiration of three minutes and a half; as soon as the compression is removed, every globule starts off like an arrow. Thus, then, the globules, separated from the action of the heart by a ligature, continue to move for a certain time, from the effects of the elasticity of the parietes of the vessel, but the circulation does not go on as before. If the femoral *vein* be tied, the progress of the globules in the vessels continues some seconds, then stops, and is transformed into a sort of come-and-go movement. Finally, if the artery and vein be compressed together, there is absolute immobility. The author has thus, by experiment, verified two important facts in the history of traumatic hæmorrhages. It is, that, in capillaries cut by a sharp instrument, the blood moves in the same direction as in the arteries and veins, namely towards the amputated extremity. This is, besides, a natural consequence of the contractility of the vascular parietes, which is no longer counterbalanced by the influence of the heart. If a part be entirely separated from the body, there is, in the same manner, a flow from the orifices of the open vessels, which is due to the same cause. The author afterwards satisfied

himself that the number of jerks of the globules in the capillary vessels is equal to, and isochronous with that of the pulsations of the heart. If this organ be suddenly removed from a living frog, the blood takes a retrograde movement in the arteries: that which it had in the veins continues in the same direction, but is notably accelerated, since the pressure to which the blood is subjected in the vessels from the action of the heart is superior to that of the atmosphere. If merely the ventricles of the heart be tied at its base, the same phenomena take place, though in a less marked manner. It is, therefore, to sum up, to the influence of the action of the heart that is owing the circulation of the blood, in the capillaries as well as in the arterial and venous trunks which they connect.

M. Girard, the celebrated civil engineer, had long ago said that, in tubes at rest, of small diameter, the walls of which are susceptible of being moistened by the liquid moving in them, there lay around the walls of the tube a stratum of motionless liquid, which surrounded on all sides the liquid in motion, and separated it from the walls of the tube. M. Poiseuille has established the same fact with regard to the blood vessels. If they be carefully examined by the aid of the microscope, it is seen that from the axis to the parietes of the vessel, the globules are possessed of very different velocities. In the axis the velocity is at its maximum. Close to the parietes we distinguish a very transparent space, where globules rarely show themselves. This space has a breadth about equal to the eighth or tenth of the diameter of the vessel. If we follow the course of the globules in a vessel, the diameter of which allows the passage of three, five, twelve, twenty globules abreast, the velocity of the globules in the axis of the vessel is the greatest; this velocity diminishes more and more as we approach the transparent space. In the axis and its neighbourhood, the globules have merely a forward movement; but near the parietes they have both a forward and rotatory movement, this last movement being more decided the more we approach the motionless stratum. Those which touch it, turn, so to say, upon it, and move forward much less rapidly than those of the axis of the vessel. This transparent part of the vessel, comprised between the course of the globules and their parietes, is owing mere-

ly to the presence of the serum belonging to the blood. Thus of the blood, which is incessantly in motion in all the vessels, neither the solid portion, the *globules*, nor even the liquid, or the *serum* touch the parietes. A layer of serum, by its immobility, protects them from the wear and tear which would result from direct friction against them. M. Weber distinguishes in the blood two sorts of globules; the sanguineous globules, which are elliptical, and the lymphatic globules, which are spheroidal; according to him, the latter only turns on the layer of serum. M. Poiseuille has since satisfied himself that cold retards the circulation in the capillaries, while heat retains it. He, therefore, infers that, in cold weather, other things being the same, the circulation is accomplished with more difficulty than in warm. Hence inflammations are as numerous in the latter as they are rare in the former season. Rheumatism of any part of the body unquestionably owes its existence to the arrestation of the globules consequent on the cold produced by the evaporation of the perspiration, when this part has been exposed to a current of air. But these considerations will be discussed in a work which the author is preparing on Inflammation, and in which the greater part of the phenomena will be based upon our physiological knowledge.

It is known that many authors, and in particular Dr. Barry, had attributed a very important part to atmospheric pressure in the phenomena of the circulation. The greater part of the sensations experienced by travellers who have ascended to great heights, were explained in this manner. To satisfy himself of the truth of these assertions, M. Poiseuille contrived an apparatus, which he calls the *pneumatic porte-object*, and by means of which he is able to examine, under the microscope, the capillary circulation in an animal, either by subjecting him to a pressure of two, three, and four atmospheres, or by reducing this pressure to two centimetres of mercury, that is to say, the thirty-eighth part of what it ordinarily is at the seaboard.* Thus the phenomena, which result from the rarefaction or extreme pressure of the atmosphere, depend upon the respiration, and not upon the capillary circula-

tion, and the movements of inspiration and expiration are, no more than atmospheric pressure, indispensable causes of the circulation, but only accessory causes of the movement of the blood in the arteries and veins.

DOMESTIC SUMMARY.

On the power of Saccharine substances in general, and uncrystallizable sugar in particular, of protecting the solution of Protiodide of Iron from decomposition.—BY WILLIAM PROCTER, Jr.—To be able to protect the solution of protiodide of iron from decomposition, and, consequently, to preserve its medicinal power unimpaired, has been a desideratum to the medicinal practitioner, as owing to the gradual but certain process of reduction of the strength of the solution, this preparation is one to which the physician is compelled to look with suspicion, not on account of any original want of power in the remedy, but by reason of the process of deterioration which is constantly progressing, often, notwithstanding the recommended precaution of keeping metallic iron in the solution.—It is true that, when this is done in a proper manner, there is a mere transfer of iron from the protecting metal to the iodine of the decomposed salt, as it is gradually effected, while its base is deposited in the state of peroxide. But even allowing this protecting power to be fully exercised, the ferruginous deposit, making the solution turbid, not to speak of the iron filings or wire, renders it inelegant, and frequently subjects the apothecary to the inconvenience of filtering the solution before dispensing it.

Thus far we have been viewing the subject within the precincts of the apothecary's store, but the most serious difficulties yet remain to be considered, viz.: after the remedy is placed in the hands of the patient. Some pharmacutists take the precaution to introduce small quantities of metallic iron into the vials of the solution before selling it, but this is by no means general. As the remedy is taken in small doses, and its exhibition sometimes continued through considerable periods of time, the frequent opening of the vial, not to say the liability of leaving it unstopped, as must often be the case, causes the decomposition to go on rapidly when it is not in contact with iron. In corroboration of these remarks, I have the testimony of one of our first physicians, who informed me that he had discontinued the use of this preparation in his practice, in consequence of its great variation in strength.

I have deemed it proper to make the foregoing remarks as introductory to the following observations and experiments, which were made with the object of discovering a remedy for the evils which we have seen detailed.

The agents employed belong to the saccha-

* He has satisfied himself that these differences of pressure have no influence upon the velocity of the capillary circulation.

rine substances. The first application of one of this class of substances as a protective agent in pharmaceutical manipulation, was by Vallet, in his now celebrated ferruginous pills.

Shortly after the formula for his preparation was published in this country, I gave a process for preparing a tincture of *protomuriate of iron*, (Vol. X. p. 272, of this Journal,) which was kept in the state of a protosalt, through the intervention of honey. It was this idea which suggested the power of the same agent in preserving the solution of protiodide of iron, and the sequel will show how far the suggestion has been realized.

As it is our duty at the time we are pursuing a course of investigation, to note *all* the phenomena that offer, perhaps it will be well to give an idea of the *relative* protecting power of several saccharine substances, so as to be able to appreciate their importance, and to ascertain, if possible, some general principle which will account for the variation in their protective power.

The substances tried were sugar of milk, manna, cane sugar, honey, and uncrystallizable sugar.

The uncrystallizable portion of honey and molasses was obtained by mixing the honey or molasses with twice its weight of alcohol. The cane sugar precipitates, if molasses is employed, and the crystallizable honey, if honey is used, and by evaporating the solution, the preparation is obtained free from alcohol. They should be decolorized as much as possible, by boiling with animal charcoal, before being used.

1st. The power of sugar of milk and manna in protecting the iodide is very slight, although they each may possess the property in a limited degree. The trials which were made, however, offer convincing proof of their inadequacy to perform the purpose in view.

2d. Three drachms of the solution of protiodide of iron was mixed with one drachm of simple syrup, and placed in a vial exposed to air and light. In a similar vial a like quantity of the solution was placed without the addition of syrup, and equally exposed to air and light. At the end of forty-eight hours the saccharine solution remained transparent, possessing its original colour, while the other had acquired a brownish hue. They remained thus exposed to air and light for two weeks—at the end of that period the unprotected solution had deposited a considerable quantity of peroxide of iron, and was strongly charged with free iodine. The saccharine solution was also coloured, but in a very slight degree, with an equally small deposit of ferruginous oxide, but we must be aware that this occurred after a full exposure to *air and light* for two weeks.

3d. Three drachms of the solution, as before, was mixed with two drachms of simple syrup, and the mixture exposed for five days to air and light. On examination, the solution

remained perfectly unaltered, not communicating the slightest tinge to starch water, or exhibiting any precipitate whatever.

The vial was then corked, and left exposed to the light. Twenty-two days after the commencement of the experiment, the first evidence of free iodine was manifested, and to this time, nearly two months from that date, the solution remains so little altered that the presence of free iodine is hardly perceptible, and the deposit of oxide equally minute; there being none whatever on the sides of the bottle.

4th. Three drachms of solution of protiodide of iron was mixed with one drachm of honey, and the mixture filtered, to render it perfectly transparent, and then exposed to air and light for three days without being the least affected. Twenty days after, the solution remained unchanged, and one month had elapsed before the slightest trace of free iodine could be detected. Two months after decomposition had progressed so tardily, that if no other protective agent existed, this would be better than iron.

5th. Three drachms of the iodous solution were mixed with one drachm of uncrystallizable honey. The mixture was treated precisely as in the last experiment, and was found to answer more effectually than the unaltered honey.

6th. Three drachms of the solution, as before, was mixed with two drachms of uncrystallizable sugar, (of molasses.) The mixture was then exposed several days to light and air without the slightest alteration. Nearly two months have elapsed since the beginning of the experiment, and not the slightest trace of free iodine, or of oxide, have been separated, notwithstanding it has been tested twenty times with solution of starch.

7th. To exhibit the protecting power of these agents more pointedly, two vials, with wide mouths were nearly filled with filtered starch water; to one was added a few drops of the protected solution, to the other an equal quantity of the unprotected solution of iodide of iron. At the end of twenty-four hours the presence of free iodine was rendered evident in the latter by the blue colour acquired by the starchy solution, whereas the former remained colourless.

8th. Thinking, notwithstanding the present protective power of the agents, that the increased temperature of summer might cause fermentation, and thus render the protector worse than useless, four ounces of the solution, protected with uncrystallizable honey, was placed in a vessel of water, the temperature of which varied from 80° to 100° Fah., for nine days. At the end of this period no signs of fermentation were evinced, and no free iodine existed in the solution. The exposure to heat was then discontinued, under the impression that if disposed to ferment, the solution had ample time to give notice of it.

It remains now to offer a formula for the proposed preparation. The strength of the following is that proposed to be adopted at the late convention on the Pharmacopœia :

Rx.—Iodine, - - - 3xi.
Iron filings - - - 3iv.
Syrup, - - - -
Uncrystallizable honey, or
Uncrystallizable sugar, - 3iv.
Distilled water, a sufficient quantity.

Mix the iodine with eight fluid ounces of the distilled water, and gradually add the iron filings, stirring constantly ; then apply a gentle heat until the solution shall have acquired a light green colour, or shall not give a blue colour to the solution of starch, then add whichever of the three protecting substances may be chosen, and continue the heat a short time, and filter. Lastly, wash the filter with as much distilled water as will make sixteen fluid ounces of solution of protiodide of iron.

When either sugar or honey is used, the colour of the solution is very little altered, while it is rendered much more palatable.

To the physician, the foregoing remarks are believed to be fraught with advantage, as it gives him the power to control the efficiency of his remedy by merely directing a quantity of simple syrup to be mixed with the solution, when he has reason to believe that it is not done previously, and increasing the dose proportionably. To the pharmacist it offers to be equally beneficial, by rendering a preparation, hitherto uncertain and inelegant, permanent in its medicinal power, and free from a sedimental deposit, which, he will admit, adds nothing to the appearance of his bottle.

American Journal of Pharmacy.

FOREIGN SUMMARY.

PHILLIPS' LECTURE ON THE PRINCIPLES AND PRACTICE OF SURGERY.—NO. III.

CANCER—(Continued.)

Medullary Form—Anatomical Characters—Chemical Characters—Distinctions between Scirrhus and Medullary Tissues—Cutaneous Cancer—Formation—Carmichael's, Adams', Cruveilhier's, Müller's, Broussais', Carswell's Opinions—Period of Life—Causes—Diagnosis—Prognosis—Exceptions—Treatment.

In examining the progress of medullary structures, we may admit three stages ; in the first, the tumour has the consistency of conglobate glands ; in the second, it is softer ; in the third, the softening is complete—it is almost semi-fluid, giving to the touch a sensation of fluctuation. A fourth period may be admitted ; it is that of ulceration of the integuments, when the tumour is near the surface. The dissection of medullary structures (says Lobstein) shows that the mass is composed of three distinct parts—cells, parenchyma, and extravasated blood. The cellular tissue, which con-

stitutes the frame-work of this structure, occupies only a very small space ; it is soft and semi-transparent. The parenchyma is presented with different degrees of consistency : in the crude period it is more or less transparent, hard, divided into lobules ; in the second period, the mass is like the brain of a young child, divided into lobules by fissures, in which blood vessels, whose texture is extremely delicate, are lodged ; in the third period (that of perfect softening,) the consistency of the mass is not much greater than that of thick pus. Between the first and second periods, medullary structures present an appearance which has been little noticed. The consistency being considerable, when the tumour is incised a milky matter is yielded, not upon compression, but upon scraping the surface with the knife, though no cells or vesicles which could have contained it appear.

Berard injected an encephaloid tumour, and found that in its substance there was a great preponderance of arterial canals, and that this preponderance was as much greater as the softening was more decided. He found that the membranous envelope presented an abundant venous plexus, interlacing with the arterial ramifications. In a section of the tumour he could not see a single venule or black point, whilst the injection freely pervaded the adjoining thyroid body. Either, therefore, there were no veins, or they were filled up with some matter other than fluid blood ; and, in fact, it was found, upon squeezing the mass, that encephaloid matter oozed out from numerous canals apparently venous. Velpeau describes a case in which the kidney was the seat of disease, and cancerous matter was found in the inferior cava. Many similar cases are detailed by Cruveilhier and others.

It is probable that the reason of the apparent absence of veins is this : the blood moves more slowly in the veins than in the arteries ; it is in the veins that the blood becomes stagnated ; the blood moving in the veins in a medullary tumour, while in this almost stagnant condition, may be transformed into a medullary mass, and the veins be, in this way, made impervious to injection. This explanation is not, perhaps, applicable to all cases, for in those of Berard there was destruction of the venous parietes ; but whether the medullary matter found its way into the veins, or whether it was already transformed in the canal, is a matter which cannot easily be determined. In many specimens of medullary tissue, Cruveilhier thought the structure represented a sort of "feutre" or felt, or cavernous erectile tissue ; that there was identity of aspect between the areolar framework of the cancerous and erectile tissue : this he thinks we cannot be surprised at, since the accidental erectile tissue is nothing else than a development of the venous capillary system ; so that there is not only identity of aspect but of affinity between the erectile and cancerous

tissue. "I regard," says he, "as a truth acquired in science, that cancer has its immediate seat in the venous system. A second variety is, says he, eminently vascular, appearing to be composed of veins flattened and flexuous, dilated into ampulla with extremely thin parietes, easily destroyed, and becoming the source either of small or large extravasations of blood. These are more rapid in their course than the hard varieties.

Chemically.—Some experiments were made a few years ago, in the laboratory of the Faculty of Medicine of Paris, upon cerebriform tissue in its first or crude stage. Treated first by cold water, it furnished a little albumen and gelatine; treated by warm water, the filtered liquid was turbid. When evaporated, it yielded gelatine and a little phosphate of lime; by means of warm alcohol a little more gelatine was obtained, but the greater part of the matter remained insoluble in water and alcohol, presenting a fibrous appearance, not unlike fibrin or gluten. Acetic acid produced considerable tumefaction of the residue. Cerebriform matter in its second stage was treated as follows:—Two hundred parts mixed with one hundred parts of distilled water formed at first a homogeneous mass; heated, it deposited a solid coagulated matter, insoluble in water, spongy in appearance, but tenacious, elastic, horny. This same coagulum, weighing a hundred and ninety-six 'fraumes,' burnt upon lighted charcoal, exhaled a smell like burnt horn, bearing the characters of coagulated albumen. The remaining water having been evaporated, no gelatinous appearance was assumed. From these experiments it resulted, that, at its first stage, it is richer in gelatine, whilst in its second stage it contains more albumen.

Distinctions.—The following are the prominent distinctions between these two classes of structures: at its perfect state of development the medullary disease presents a milky white pulpy matter, the surface studded here and there with red spots; scirrhus presents a lard-like mass, intersected by dense white bands. The medullary tissue presents many small arteries, becoming still more numerous where softening is decided; in some points there is actual extravasation, and when ulceration takes place, hæmorrhage follows. Scirrhus has very few vessels, extravasation is very unfrequent, and when ulceration occurs, hæmorrhage seldom happens. Medullary tissue often breaks down the walls of veins, and may get into the canal, which it may distend; we have no instances of scirrhus following a similar course. Medullary tissue may affect all organs; the seat of scirrhus is much more limited. Medullary tissue may acquire great bulk, and when it softens, the mass becomes elastic. Scirrhus rarely acquires great bulk, sometimes seems to produce atrophy of parts, is not well rounded nor elastic. Medullary matter may exist

in many organs at the same time; scirrhus is generally confined to one point. Medullary tumours may be developed at an early period of life; scirrhus is rarely seen before adult age. Medullary tumours may be long in contact with the skin without adhering; scirrhus is soon adherent. Medullary tumours ulcerate very rapidly, destroying life often in a few weeks; scirrhus ulceration frequently continues for months, or even years.

Cutaneous Cancer.—There is a variety of cancer which commences in the skin. A tubercle or wart is usually presented. Scarpa examined the tissue of these tubercles under a lens, and ascertained that the tissue possessed the characters of scirrhus, but we shall in many cases vainly look for the scirrhus base which is usually found in scirrhus ulcers; besides this, they are usually very long in affecting the neighbouring glands, and are often completely cured by extirpation. These cancers take a different course from those we have been considering: they commence with an ulcer, and gradually acquire a scirrhus fundus. They are most frequently developed about the head and face, about the areola of the nipple, the scrotum, the vulva, the rectum.

Formation.—Of the mode of formation of carcinomatous structures, we are profoundly ignorant. Carmichael believed it to be a living being, enjoying independent existence. Adams' opinion was, that the essence of cancer resided in the presence of a hydatiform animal, which he called *hydatis carcinomatoso*. According to him the white septa which intersect, and the membrane which surrounds scirrhus, are living cysts—*tænia hydatidea*. The proof that this hydatid was living, is, that it was like those of sheep; that it shrivels and takes a granular appearance when an incision is made through the tissue, and that this does not happen in some hours after removal, when the breast is cold. He believed there were three kinds, serous, gelatinous, and sanguinolent. Hodgkin sought to prove that the presence of a serous membrane, having a cystiform arrangement, is necessary for the production of cancer; the existence of the former, he believes, precedes the latter. The objection which must be made to this view is, that it is too exclusive; there are certain carcinomatous structures in which a cystiform character may be shown; there are others in which it is impossible to demonstrate any thing of the kind. The cancer areolaire of Cruveilhier is "essentially constituted by the transformation of the affected tissue into an areolar fibrous structure, filled by a kind of transparent gelatinous matter." He thinks it makes no exception to what he regards as a general law, "that all organic transformations are exclusively the result of a successive deposition of morbid products in the cellular element of organs." He believes it is rarely manifested, successively or simultaneously at a great number of

points, whilst Müller believes that this areolar disease is ordinarily accompanied by general affection of the system, and that the patient dies with similar visceral disease. Cruveilhier believes it to be developed most frequently in bones, in the rectum, the uterus, the ovaries, the cæcum, the small intestine, but no where so frequently as in the stomach. Müller's opinion of the cystiform or cell-like arrangement of this form of cancer is somewhat different:—if we examine a very thin slice of this matter with a power equal to about 400 diameter, we distinguish a granular matter associated with cells or cysts; these cells are not single, but within the first we see a second, with which the first seems to have no direct connection, "like one pill-box within another." The smaller cell contains a dark yellow parietal nucleus, which seems in contact within, perhaps arising from one side of the cell. He believes these cells, or cysts, arise from the granules; that they enlarge, burst, and are succeeded by others, and that in this way the mass increases; he applies, in fact, the opinion of Schleiden. According to him, each cell in vegetable development contains a portion of starch, which is capable of being converted into nutritious matter. This conversion having taken place, a dark spot is perceived in the coats of the cell, and from this spot a new cell is seen to be protruded; accordingly, this spot being the germ of the cell, it is called by Schleiden the cytoblast. The new cell, when generated, gives birth in its interior to new cytoblasts, which again generate new cells, and thus a series of cells is produced, one within another, until the external one is ruptured, and its contents are enabled to escape, and thus to obtain their natural development. The newly formed cells are of extreme tenuity, but new matter is afterwards deposited within the interstices of that originally formed, until they gradually acquire firmness and consistency. Without knowing any thing of this discovery, Mr. Gulliver showed me this arrangement in a cancer of the breast, removed from a patient of fifty-five, by my colleague, Mr. Stafford. He found considerable difficulty in determining whether the granular masses succeeded to the cysts, or *vice versa*, though he inclined to the former opinion. I have examined many other cases, but without discovering a similar arrangement. The patient in Mr. S.'s case died of pneumonia, and no similar deposits were found in any of the viscera. Mr. Gulliver's notes on the structure in this case, are as follows:—"Ultimate structure of juice, molecular; globules very variable in size; many oil-like, floating freely in fluid; many contained in transparent cysts of pretty large size, very variable in shape: some oval, others globular; molecules and cysts contained in an interlacement of cellular tissue. Certainly one cyst inclosing others. After maceration in muriate of soda, one oval

cell seen to enclose two others with a nucleus (intervening parts granular;) length of large cyst 1-400th of an inch; breadth 1-555th; circular nuclei 1-5333d." Mr. Gulliver is about to submit to examination a large number of specimens of cancer, from which he expects it will result that this cellular form will be very prevalent.

Broussais and his followers believe that cancer is a consequence of inflammatory action; "all inflammatory and sub-inflammatory action may determine cancer." The authors of the Dict. de Med. art. Cancer (Breschet and Ferrus,) maintain that cancer succeeds to irritation or inflammation, and cannot be developed without being preceded by them. This irritation or inflammation determines the deposit of coagulable lymph, which may harden and constitute the nucleus of scirrhus; inflammation may attack this nucleus and disorganise it; but in its passage from an organic to an inorganic state, there will be a detritus from which cerebriform matter results; if it be mixed with blood, or new capillaries be formed, fungus hæmatodes may result. In this theory two distinct inflammatory actions are required or invoked; the one primitive, which may be only inflammatory irritation, but determines the formation of scirrhus; the other secondary, by which it is changed into medullary matter. Of this, I am not aware that any proof can be given; we are not therefore justified in assuming that scirrhus results from inflammatory irritation. In fact do we find scirrhus oftenest where irritation is most decided? Is it in abandoned women that cancer of the uterus is most frequently seen? Is cancer of the mammary gland more common in those women who have nursed many times than in those who have never had children to nurse? I believe not. If we examine scirrhus soon after it is detected, we do not find the smallest vestige of inflammation, yet it may increase. Now, if it can enlarge without inflammation, may it not be formed without it? In as far as concerns the second degree of scirrhus, or its passage into the period of softening, it seems to me that an irritant action must be admitted. What else than inflammatory irritation could produce the change of texture?

The following reasons seem to me conclusive against these tissues being a consequence of inflammation: that they are manifested by characters essentially different from those of inflammation; that they are not necessarily preceded by inflammation; that inflammation does not explain the accidents they determine, nor the disorders which characterize them; that they never seem to arise simply under the influence of causes of inflammation, and are not manifested by the same symptoms; that they are preceded by the formation of an organized tissue, whose nature and aspect is different from that of tissues which have been altered by inflammatory action; that the for-

mation of this tissue, either in masses or infiltrated in the cells of organs, is explained, naturally and simply, by an alteration in nutrition, which is characterized by the secretion into the diseased part of a substance which is at first organized and afterwards softened, disorganized, and, at a certain period of its existence, reduced to a pulpy or detritic mass; that scirrhus and cerebriform matter is identical, or nearly so, in all tissues, whilst inflammations are not perfectly the same in all tissues. Inflammation being a disease of tissues, is modified by their vitality and their organization; whilst cancer, resulting from the primitive formation of a morbid substance which constitutes it, is never primarily subjected to the particular state of the organ in which it is developed or deposited.

Dr. Carswell says "that this substance (cancerous matter) exists in the blood." That the deposit is a consequence of a modification of the blood, is evident from the particular mode of deposition; from the existence of this matter in the vessels ramifying in carcinomatous matter; from its being found in vessels having no direct communication with an organ affected with cancer, and in blood which has been effused into the cellular tissue or upon the surface of organs. The divisions of the vascular system in which it has been found are the venous and the capillary. Whether it be, as Dr. Carswell observes, "that the presence of an organized product in the blood can have no other origin than the blood itself, and that it cannot be introduced into this fluid by absorption; whether it be a product of secretion, and not existing in the blood; or whether, as in the two cases described by Berard, the matter got into veins passing through the carcinomatous mass by a destruction of their parietes, may be a matter of question. Velpeau thinks that the blood in a vein may be converted into carcinomatous matter. Cruveilhier, at the same time that he believes the cellular tissue to be the structure in which cancerous matter is deposited, expresses his conviction that all new products, all morbid alterations, are formed at the expense of the venous capillary system. Laennec says it is a lesion of structure; Cruveilhier, Andral, Lobstein, and Carswell, that it is a lesion of secretion of nutrition. There are, says Carswell, several organs where means are afforded for ascertaining the seat, origin, and mode of formation of cancer; but it is necessary to observe it at an early period of its existence. Investigated in this its first stage, we ascertain, with greater or less facility, that this substance becomes manifest to our senses, either as a production of nutrition or of secretion. In the former case it is deposited in the same manner as the nutritive element of the blood, enters into the molecular structure, and assumes the form and arrangement of the tissue or organ into which it is introduced; in the latter it makes its appearance on a free

surface, after the manner of natural secretions, as on serous surfaces in general. It may be found not only in the molecular structure, and on the free surface of organs, but also in the blood. He believes that an organ is often not at all enlarged by the deposition of carcinomatous matter, and that it is a mere exchange for an equal quantity of the natural tissue, which has been absorbed in the usual way.

Period of Development.—Although what is termed the critical period of life, or that included between 35 and 55, be the time of life when carcinoma is most frequently developed, it may also occur earlier or later. In the earlier periods of life, the variety usually seen is medullary; it may affect the eye or it may affect the skin. A few months ago, I had a cancer of the perineum, in a young woman of 23. I have at present, cancer of the rectum, in a lad of 17. Not long since, a young woman died, at the age of 21, from cancer of the uterus. It is certain that women suffer from the disease more frequently than men.

Causes.—Of the cause of cancer we are utterly ignorant. We have no evidence to show that cancer is contagious; cancerous ichor has often been inoculated, but I am not aware that it has ever produced cancer. Men have cohabited with woman suffering from cancer of the neck of the uterus, but without propagating the disease. Neither have we any evidence to prove that it is hereditary; though the popular opinion favours that belief. And again, with regard to the depressing passions, also assigned as a cause, we see cancer developed in persons habitually cheerful, in whom no function has been seriously injured, and who have enjoyed good health; and in whom no external injury has been inflicted upon the organ which is the seat of the disease. With respect to the cessation of menstruation, we cannot deny that a tumour, long indolent, may rapidly increase under it; but, on the other hand, how frequently do we see cancers even in a state of ulceration before the cessation of this function, and even while it is regularly performed.

Some persons believe that a mysterious unknown condition, termed *diathesis*, excites the development of cancer; they believe that there exists, in certain cases, an internal disposition sufficient to produce cancer; it is, say they, the true and unique cause of the reproduction of cancer after extirpation; it is upon it that the simultaneous or successive development of the disease in several organs, far removed from each other, depends. According to them, this diathesis may exist many years, or the whole of life, without being manifested by any external or internal sign. Bayle goes so far as to say that cancer is never a local disease, though determined by an external cause; that it is to this diathesis that cancers owe the property of reproduction twenty years after extirpation, though, during the intermediate time, the patient has seemed to enjoy good health.

To consider, as the reproduction of extirpated cancer, that which is manifested twenty years afterwards, whilst, during this long interval, the patient has had seemingly good health, is too great a stretch for my imagination. It establishes incontestably, if admitted, that cancer is an incurable disease.

That cancer is a disease whose local manifestation is secondary, is highly probable; thus twenty women, at the age of 45, placed under apparently exactly similar circumstances, shall receive a similar blow on the breast; in one case cancer may succeed to the injury—contusion happens in all, but the one has a disposition to the disease, and the disposition is excited into action by the injury. There are certain “constitutional” diseases which we know how to excite—syphilis and scorbutus; there are others which we know not how to excite at will—phthisis and cancer.

Diagnosis.—With all the knowledge we can bring to bear upon the subject of cancer, the diagnosis is still in many cases a matter of great difficulty. When the tumour is indolent, and ulceration has not taken place, it is often impossible to pronounce upon it with any certainty. Though accessible to the eye and the touch, and not very deep seated, we frequently cannot positively determine, before extirpation or puncture, the morbid structure of which it is composed: neither the length of time it has existed, nor the obscurity of the causes by which it has been produced, nor its density, nor its mobility, furnish certain indications upon which to determine whether the tumour be formed of scirrhus, or medullary, or fibrous or fibro-cartilaginous matter, or whether it results from a simple chronic induration of the affected organ. But if our power of diagnosis be not sufficiently precise to make out these anatomical peculiarities, we can in most instances distinguish, from all others, scirrhus and cancerous productions, and bring to bear a sufficient number of circumstances to justify an operation. Scirrhus tumours, externally situated, may be confounded with all others which exist without heat, change of colour, and fluctuation. The presence of one of these signs suffices to exclude all idea of scirrhus or unsoftened medullary matter, and acute lancinating pains, occurring with irregular intervals, can rarely be confounded with that determined by ordinary inflammation. The situation of aneurisms in the course of arteries, and their pulsation isochronous with the motion of the heart, suffice even when they are firm and not fluctuating, to distinguish them from scirrhus. Fatty and encysted tumours present either an inelastic softness, or an obscure and imperfect fluctuation, which distinguishes them from organic products, and they are commonly not developed in those situations where scirrhus is usually found. Certain dense fibrous or fibro-cartilaginous cysts, containing either hydratids or suetty matter, may sometimes

mislead, but then such tumours are not common externally, and they always present an elastic resistance, and usually an obscure fluctuation, by means of which we might distinguish their true character. It is generally more difficult to distinguish scirrhus from fibrous tumours, which are not very unfrequently developed in the mamma, and are often found in the uterus. But these fibrous tumours have generally a smooth surface, rounded and regular forms, neatly separated from adjoining tissues, which contrast with the knotted surfaces, close adhesion, and great density and weight of scirrhus. If a tumour remain long, hard, and apparently inert; gradually becomes the seat of lancinations, which increase in frequency; if soft, and apparently fluctuating points appear, under which the skin is thinned and reddened, there can be no doubt that malignant disease exists. It may happen that a tumour exists in the breast, and that a sanious fluid escapes from the nipple. This very grave symptom may however mislead; I have had the history of two cases in which the tumour of the breast had succeeded to a blow; after a time a sanious fluid flowed from the nipple, the tumour was extirpated, and was found to be formed by a clot of blood succeeding to the injury: the clot broke down, and its more fluid portion found its way into the lactiferous ducts. Even when ulceration has occurred, when we see an ulcer with a scirrhus base, everted edges, ichorous, fetid, acrid secretion, with a tendency to hæmorrhage, these circumstances are not peculiar to each species of cancer. Cancer of the face is often altogether indolent, and the scirrhus base is often not found elsewhere than in cancer of glandular structures. The everted cut edges, the tendency to hæmorrhage, fungous vegetation, are seen in ulcers which are not cancerous, and all cancerous ulcers do not yield a fetid sanies; therefore these so called characteristic signs have only a relative value. The specific odour of the pus, it is important to notice, the eroding character of the ulcer, the tendency to swell of the neighbouring glands, and the appearance of suffering impressed upon the countenance, the pale, dry, parchment-like skin, and the tendency to reproduction. In spite of all these signs, it must be borne in mind that, in an affection the proximate cause of which is unknown, there can be little certainty.

Prognosis.—Can cancer be cured? I believe there can be no question that a cancerous tumour is not susceptible of resolution; we know no medical agent capable of eradicating it from the constitution. We do not think it impossible that ultimately such means may be discovered. We have a panacea for syphilis; we may, by change of climate, now and then arrest the progress of tubercles. With regard to cancer, its progress has been arrested; and after ablation in some cases, few though they be, the disease has not reappeared; if this be

so, we must admit that the cause under the influence of which it was principally developed has been destroyed or arrested in the interval. The cases described by Mr. Hill, eighty-eight in number, I do not propose, to consider, because they are so opposite to the results of general experience, but of these eighty-eight only five were tumours of the breast, and of these five only two were apparently cured. Those of Recamier are to me little more satisfactory. Monro operated, by ablation, in sixty cases of cancer; at the end of two years four only had not suffered relapse. Scarpa practised his profession very extensively for sixty years, and only knew three cases in which the disease had not reappeared. Boyer removed a hundred cancerous tumours; in four or five cases only was there an apparent cure. Besides those operations, I have collected, from the experience of eminent surgeons, 703 cases, in which the breast was extirpated, and there were only twenty-two apparent cures. Still we must not regard every case where a second cancer tumour is discovered, as a relapse, because it is possible, of course, that they existed before the operation was performed, and that they were developed under the influence of causes similar to those which had occasioned the external disease. It is maintained by some persons, that the cancerous matter is softened, absorbed, carried into the circulation, and afterwards deposited in various organs; the absorption is not however sufficiently made out to be considered an established fact in science. If it be admitted as possible in external cancer, it must be equally possible when it is deposited in internal organs.

If cancer be reproduced after extirpation, it happens in different ways; sometimes the process of cicatrization is interrupted, fungous points or tubercles are developed, and a cancerous ulcer is soon produced. Sometimes the cicatrix is completed, after some time it is raised by a tumour, is destroyed, and the disease reappears. Sometimes it happens that the disease reappears in the lymphatic ganglia related to the part where the disease was originally seated. Those of the axilla, when the mammary gland has been affected, those of the pelvis, when the testicle has suffered. It may, however, occur in parts perfectly unconnected with the original seat of the disease.

In the relapses, the nature of the tissue sometimes changes, but usually it is the medullary structure. The interval comprised between the destruction of a cancerous tumour, and the reproduction, is very variable; sometimes it is very short, at others it is very long. But we know not why, no more than we know why in rabies so long an incubation sometimes happens.

Exceptions?—At a time when the impression is almost universal that cancer is a disease beyond the reach of art, and when, consequently, the disinclination to resort to extirpa-

tion of the morbid structure is daily increasing—when at the same time no one is disposed to deny that occasionally, though very unfrequently, cases occur in which reproduction does not happen—it is interesting to inquire whether we have any signs by which to distinguish a curable from an incurable cancer. Within a few years I have had opportunities of examining ten cases in which cancer commenced in the cutaneous integument, or portions of the mucous tissues nearly connected with it: one was a case in which the disease commenced in the perineum, and extended to the vagina; two were cases of cancer of the lip; one was a case of cancer on the penis; two were chimney-sweeps' cancers; in one, the disease had begun in the areolar tissue of the mammary gland; in one the rectum was the seat of the disease; and in one, the inside of the thigh. These cases were examined after death, three only dying from the immediate effects of the disease. In no one of these cases was a similar deposition found in any viscus. Supposing further experience confirm this as a common, though not a constant circumstance, it will still enable us to relax the rule which is now so stringently applied as to the unvarying fatality of the disease. It will be an evidence of the probability that cancer commencing in the cutaneous integument does not commonly, unless at an advanced period of its existence, pervade the system, so as to constitute a constitutional disease. Few surgeons of much experience have not seen cases in which cancer of the lip and cancer of the penis have been cured; and these cases no doubt depend upon the absence of constitutional infection. With regard to the other varieties, so much may be said, that though few, yet undoubted cases exist in which the disease has been extirpated, and no symptom of relapse has been manifested at the end of many years. In these cases, either no deposition of the carcinomatous product has occurred, the disease being local, or if deposited, its further development has been arrested. It is very important, therefore, to ascertain, by the aid of the microscope, and its general characters, the particular structure of each of the tumours which may be removed, as it may enable us to establish that certain other varieties of carcinomatous structures have little tendency, at least in the earlier periods of their existence, to infect the constitution generally, and that we may therefore recommend the removal as a probable curative means.

Treatment.—Two things must be necessary to cure cancer; the destruction of the cause, and the extirpation of the local disease. In surgery we extirpate the local disease, but we know that to obtain a cure both the local and general disease must be extirpated. If surgery obtains such inconsiderable success, it is because the extirpation of the local disease

does not imply extirpation of the constitutional taint. In medicine, the success is even less, because neither the general nor local disease can in the present state of our knowledge be thus extinguished. The use of remedies has changed with the changing opinions as to the nature of the disease; when cancer was held to be a voracious animal, the hunger of this devouring vulture was appeased by placing upon the part slices of raw meat. The school of Broussais regarded it as inflammatory in its origin, and during its progress used the antiphlogistic treatment. Those who regarded it as a particular virus, employed such means as they conceived to be likely to neutralize that virus. These, and a great number of other fanciful means of treatment, have been employed and abandoned. In the present day, the medical treatment of cancer is directed to three main objects—to resolve the cancerous tumefaction, to alleviate the pain, and to change the constitution so far as to remedy the cachexia.

To attain the first object, mercurials, arsenicals, alkalies, iodine, and cicuta, hydrocyanic acid, iron, the *Calendula officinalis*, blood-letting, and emollients, have been extensively used, and many fancied cancerous tumours have seemed to yield under them.

To quiet pain, narcotics, such as opium, belladonna, aconite, and hemlock, have been much used; they may also be employed locally when the tumour is external. For many years, hemlock was conceived to have a specific action upon these tumours; indeed, the eulogiums of Storck led men to expect much from it. It has been much tried, but has never afforded any encouragement to hope that it has any other effect than that of lessening pain. Within a few years it has been very extensively employed in the practice of Recamier, in cases of cancer of the breast, the uterus, the testicle, the liver; and he believes with great success. He found the effects to be materially influenced by the quantity of food taken; if much food were taken, the action was null; if the patient were rigidly dieted, the action was very decided. His mode of administering the remedy was to take a dose morning and evening two hours before the first and the last meal, beginning with half a grain, and not exceeding six grains; this dose is continued for a fortnight, it is then increased to twelve grains each dose; this is continued for three or four weeks. Not more than one-third of the usual quantity of food should be taken, and it should be divided into three meals. Towards the end of the treatment the hemlock is gradually diminished, and the quantity of food as gradually increased.

M. Gama exhibited cicuta in combination with calomel—four parts of the former to one of the latter; one-grain pills are formed; at first, one is exhibited morning and evening, then two, increasing the dose one pill daily; in this way the dose has been increased to

twenty-five, thirty, or even forty daily. As might be expected, the effects of this treatment are very decided; sometimes abundant salivation, at others purgation. If the patient be much pulled down by it, the treatment is suspended for a time. It is generally associated with antiphlogistics and the *cura famis*. In certain obstinate indurations, and certain unhealthy cancerous-looking ulcers of the head and face, it succeeds sometimes very well, but I have never seen any thing to assure me that it will cure true cancer tumours.

The means employed to remedy the cachexia are similar to what would be employed in cases of chronic inflammation; but they do not succeed; the food should be more vegetable than animal; irritating and stimulating substances should be avoided. Though the *cura famis* may succeed in cases of chronic inflammation it will not here, though it may be very useful as a principal or auxiliary remedy. Arsenic has been much used. Justamondy regarded it as a specific. Hill thought it a very valuable remedy. Bielt and Thomson employ it in the form of an iodide. Allman has much used the iodide of potassium, and considers it very efficacious in cases of cancer of the face, the breast, and the uterus; he used it much externally; his formula was half a dram to a dram of the iodide, to an ounce and a half of lard. Mercury, Rust says, has succeeded in his experience in many cases of cancer of the lips and the tongue. In cases of true cancer these various means will, almost if not altogether, disappoint those who may employ them. And this impotence of all means of treatment, internally employed, induced men to attempt to remove the disease by means directly applied to it. These may be resolved into three; compression, caustics, and cutting instruments. It is desirable that you should perfectly comprehend that the action of these agents is strictly local, and that they do not strike at the root of the disease. Acting upon this opinion, some persons have rejected, in all cases, the assistance of surgery, and have urged that the patient should not be subjected to the pain and danger of a bloody operation, when this operation is at best only palliative. Besides, it has been observed in some cases that the wound resulting from ablation of the tumour becomes a cancerous ulcer, and the progress of the disease has appeared to be accelerated. Nevertheless, although I feel all the weight of these reasons, I think that the operation should in many cases be attempted, because cases of apparent cure do occur, and many cases of great relief. The extension of life which it procures is not dearly bought at the expense of the suffering of an operation. Of course, where the diseased mass cannot be completely circumscribed, an operation is contra-indicated. There are other circumstances which should make us pause; for instance, if hereditary transmis-

sion seemed probable; if the constitution seemed deeply imprinted. In such cases the disease would infallibly reappear and the suffering of the operation would probably hasten a fatal termination. It is contra-indicated when adhesions are so extensive as to render complete extirpation very improbable. There is another most important caution I have to give; do not operate, if you can avoid it, when the disease is making sensible progress. Its progress should be suspended. To this, some persons would demur; they would say, do not operate when its progress is suspended, because that suspension may last for years. But this confidence in the suspension is dangerous; often, a cancer, the progress of which has appeared to be arrested, suddenly lights up and proceeds rapidly. Nothing is, therefore, more reasonable than to profit by this period of repose to operate, instead of exposing the patient to the dangers of exacerbation. It is necessary to distinguish between the cancerous cachexia, which is a contra-indication, from that state of marasmus and feebleness, brought about by pain, which does not always contra-indicate operation. The general indications it is impossible to give; a calculation must be made between the strength of the patient and the dangers of the operation. It is also necessary to look at the age of the patient, for if, in the common course of things, he have not long to live, it is a matter of grave question whether a slight extension might not be too dearly purchased by operation. Still, advanced age does not absolutely prevent operation; and this is especially the case in those of the lip, tongue, and cheek.

Compression.—Dr. Young published his minutes of the cases of cancer and cancerous tendency, successfully treated by compression, in 1816. Sir Charles Bell was urged by the Governors of the Middlesex to give it a fair trial; he did, and reported that the compression of cancerous tumours, ulcerated or not, is injurious, and accelerates the progress of the disease. Fifteen years ago Recamier commenced extended experiments upon compression. In the first volume of his work, he says, a hundred patients presented themselves with cancer, sixteen seemed incurable, and were not treated; of the remaining eighty-four, thirty have been completely cured by compression; twenty-one have been improved; fifteen have been removed by ablation alone or combined with compression, and six by compression and cauterization; in the remaining twelve, the disease did not yield. That chronic tumours of the breast may be dissipated by carefully-made compression alone, or associated with blood-letting, is probable; that certain uterine tumours may yield to pressure I can believe; but I have never seen any thing to convince me that compression will cure a cancerous tumour. It is quite true that a can-

cerous tumour may apparently give way under well-applied pressure, but then a careful examination will show that the morbid structure still exists flattened, and the apparent diminution in the size of the organ is owing to an absorption of the cellular tissue, which always happens under pressure.

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Contributions to the Pathology of new born Infants. By THOMAS H. BURGESS, M. D.—Apoplexy and asphyxia in new born infants require such speedy and opposite treatment, that their diagnosis is extremely important. Dr. Burgess undertakes in this paper to illustrate apoplexy, intending on some future occasion to pursue the subject by observations on asphyxia. He details a case of birth in which there was some, but not unusual, delay, which would ordinarily lead to little fear of injury from mechanical causes.

The child cried loudly after the birth of the head, and again after the expulsion of the body; some mucoritus was discharged from the mouth; some food was taken, but within the first hour the hands were observed to be of a mottled, slaty colour. Dr. Burgess, when sent for, found the child of a blue colour all over, except the nose, which was pale and cold. The infant moaned feebly; the mouth was drawn aside; the fingers were clenched; and after a little blood had flowed from the left nostril, the child died.

Serous and bloody effusions were discovered within the skull and under the scalp, which the author minutely details.

A large quantity of serum was also effused into the pleural cavities of the chest.

After noticing the opinions of various writers, the author is inclined to think, that in this case the most probable cause of death was one suggested by M. Cruveilhier, that the uterine contractions exercised a fatal compression on the umbilical cord contained within the womb.

After recounting the morbid appearances commonly observed in similar cases, Dr. Burgess says, "none of these authors have noticed the sanguineous effusion *beneath* the arachnoid, and the engorgement of the choroid plexis, both of which conditions obtained in this instance."

The remedy, according to all authors, is blood-letting from the cord, which is directly opposed to the treatment required in asphyxia. The characteristic mark of infantine apoplexy is, according to M. Gardien, and the author of this paper, the livid hue of the body; while in asphyxia, the infant comes into the world, as Baudelocque says, "exsanguine." — *Lond. Med. Gaz.*—*Trans. Royal Med. and Chirurg. Society.*